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ABSTRACT

Although consensus on the meaning of depression exists, theories vary widely regarding its source. Depression is essentially an affective disorder; however, because the assessment of sadness is difficult, most psychological theories of depression have focused on some nonaffective component of depression, such as activity level, cognitive processes, or interpersonal factors. The strength of these theories lies in the substantial body of therapy research which has been generated. Although new cognitive and behavioral therapies are effective with depressives, efforts to demonstrate that the source of effectiveness is in the process of the theory are usually unsuccessful. Useful theory-based research must monitor personal characteristics and stressful events in the absence of depression, distinguish stressful events which are independent of or dependent on personal characteristics, and distinguish between personal characteristics which increase the likelihood of depression from those which merely increase the likelihood of a depressive diagnosis. (NRB)

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REFLECTIONS ON PSYCHOLOGICAL THEORIZING ABOUT THE ETIOLOGY OF DEPRESSION

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Reflections on Psychological Theorizing About the Eticlogy of Depression

Paul H. Blaney, University of Miami

Presented at the American Psychological Association Convention, Montreal, 1980,
As part of a symposium: Depression Research 1970-1980: A Decade of Progress?
William M. Batsel, Jr., Chair

I have taken it as my task to provide something of a survey of theoretical trends. Time does not allow me to recount the various positions in any detail, and I don't know how much sense my paper will make to those of you who lack any familiarity with the theoretical positions I'm addressing. My summaries of them will fail to do justice to their richness -- my goal is to emphasize what I view as critical theoretical undercurrents.

Although there is reasonable consensus on what is meant by depression, there are few obvious clues in the phenomenon itself regarding its source. Obviously, depression is related in some way to the cry of a baby who is hungry or abandoned. Obviously, depression is related to the experience of grief, or more generally to usual responses to extreme adversity. These are, I believe, the only obvious clues, and they don't get us too far, since they can too easily be accommodated by diverse theoretical perspectives. It is one thing to say that depression is related to grief, quite another to specify the exact nature of that relation.

The reason that I take the grief-depression relation to be obvious is that I believe that we must define the core symptom of depression as sadness. In other words, depression is essentially an affective, emotional disorder. This may seem obvious, but the fact is that existing psychological theories of depression have tended not to take the affective element in depression as their primary point of reference. If one were to delete sadness from the list of

symptoms of depression entirely, most, perhaps all, existing theories would remain remarkably unruffled. Their primary contact is with some nonaffective component of the depressive cluster. Affect seems to be an afterthought.

Why the inattention to affect? I can think of two reasons. One is that affect is so basically subjective, experiential. Of all aspects of depression, sadness is perhaps the least easily assessed in an objective fashion. We can ask a person how sad he is, but even there we may be getting an inference based on his having observed the presence of nonaffective symptoms of depression in himself. We can take note of it if a person is crying, but that is a behavior which has too low a base rate to be of much use. There are two other indicants the use of which has been underexploited I believe: sad facial expression, and suppression of smiling and laughter in response to humor. Both have been considered but only rarely.

The second, more basic reason for the neglect of affect is the anemic state of the psychology of affect itself. When one reads in the psychology of affect, one sees a field which exists largely on notions borrowed from other areas of psychology, a field which may be too immature to be borrowed from in the study of clinical depression. For instance, Schachter and Singer's (1962) popular model of emotion relies largely on cognitive and physiological variables. It is a theory of emotional phenomena, but it is not an essentially emotional theory.

My discussion of the theoretical developments of the past decade is designed to call attention to the fact that theory building is often a process of borrowing, of drawing of analogies, of taking notions which have been useful in one area and trying them on for size in a new area, perhaps after a bit of custom tailoring. And that is what one sees in the theoretical work on depression in the past ten or fifteen years: one sees theories which are the result of some existing theoretical framework in another area having been, on the basis

of some overlap between its original purview and the symptoms of depression, adapted as a theory of depression.
The overlap has usually been with the cognitive, activity-level, or interpersonal aspects of depression, not, as I mentioned, with the affective aspects, and in the case of the psychological theories, not usually with the vegetative aspects either.

Turning first to theories which connect with depression in terms of activity level, we find those which were inspired by basic research and theory on learning processes. Here, I am referring of course to the writings of Lewinsohn (e.g., 1974), of Ferster (e.g. 1974) and to Seligman's pre-1978 writings (e.g., 1974) Each took as his point of departure some paradigm for suppressed responding in laboratory animals, and, resting on the fact that many depressed persons are inactive, applied the research paradigm to clinical depression. There are two problems with this approach. First, while inactivity is seen in some depressed persons, hyperactivity may actually be more prevalent. Second, even to explain the inactivity of the depressed person is not to address the sadness, that is, the central affective component to depression. This, I assert, was a severe limitation at best. In fairness, it should be noted that Seligman (e.g., 1974) noted similarities between laboratory helplessness and depression other than inactivity, but none involved a truly affective variable. Also, some of the experimental research on helplessness and depression did employ self reports of affective state, but, as I have noted eleswhere, (Blaney, 1977) these studies used manipulations which were quite ambiguous from a theoretical point of view. Finally, Lewinsohn and his associates (Lewinsohn & Grosscup, 1979) have recently addressed the affect issue as it relates to their model; in their language, the question rests in the relation between <u>reinforcement</u> (the laboratory-based, response-defined variable) and <u>reward</u> (the effective variable). Apart from noting historical precedents for assuming such a linkage, Lewinsohn et al. argued the linkage on the basis of between- and within-subject

associations between depressive state on the one hand and frequency and affective valence of pleasant or aversive events. The relevance of this research to the reinforcement/affect question is limited at best, since it employs an operationalization fo reinforcement which may be influenced by affective state. In my view the leap from theories of responselessness to theories of depression has not been made very convincingly.

Turning next to theories which connect with depression in terms of cognitive processes, the two most influential are: Beck's (e.g., 1974) and the reformulated helplessness model of Abramson, Seligman, and Teasdale (1978). Thus we see an attempt to treat as central the cognitive components of depression, and to borrow and adapt what is known about cognitive processes. The differences between Beck's and Abramson et al.'s reflect the differences within psychological theory regarding cognitive processes from the mid-sixties, when Beck formulated his model, to the late seventies, when Abramson et al. formulated theirs. Beck, strictly speaking, did not borrow from an existing psychological model. He wasn't a psychologist and, even if he had been, it's not clear what there was at the time to borrow from. His model seems most clearly rooted in the common-sense assumption that normal, nondepressive thinking is logical, nondistorted thinking--an assumption which, incidentally, researchers evaluating the von Domarus model of schizophrenic thought disorder had already discredited (cf. Chapman & Chapman, 1973). And by now we know that depressives' perceptions of reality are sometimes more accurate than those of "normals" (e.g., Alloy & Abramson, 1979). Between the mid-sixties and the late seventies, a model regarding attributional processes in success and failure had become influential, and it was that model which Abramson et al. adapted for their model of depression. Others were similarly inclined at about the same time.

As an aside, it should be noted that there is one recent model which

could be seen as relating equally to the responselessness and to the cognitive aspects of depression: Rehm's (1977) self control model. As above, it is an adaptation of an existing model (Kanfer's) but in this case that model is best classified as cognitive/behavioral. The fact that Rehm's model, unlike those above, does not owe its existence largely to some surface similarity between a single symptom of depression and the purvue of an existing theory may be its unique virtue or its greatest weakness, depending on your point of view.

Turning now to theories which focus upon interpersonal factors, things become less tidy, less easily summarized in a minute. The reason is clear: there is no single interpersonal style or manner which is symptomatic of depression, in the sense that say, pessimism is a cognitive symptom of depression. Some clinical writers have described depressives as hostile, some have described them as dependent, others as detached. There is no consensus. The theoretical work has been devoted to attempts to discern commonalities among depressives vis a vis their interpersonal style. In a sense, the starting point has been an attempt to identify what the depressive symptom is in the interpersonal sphere. The three major suggestions have been that the depressive is interpersonally aversive (Coyne, 1976) that the depressive is ingratiating (Forest & Hokanson, 1975), and that the depressive lacks social skills (Lewinsohn, 1974).

If a consensus is reached, and if history repeats itself, we can expect that one more theories of depression will emerge as models which seem address those interpersonal patterns are adapted for use in attempts to account for depression. Actually there are instances of this already, though they are unabashedly speculative given the lack of consensus regarding any interpersonal "symptom" of depression. An example is Coates and Wortman's (1980) suggestion that the purported reactions of aversion that depressives arouse in others can be explained by adapting Lerner's just world hypothesis.

I suspect that the tone of my review thus far may seem somewhat cynical, as if I am trying to trivialize the work of these theorists, so let me hasten to disavow that intent. The theoretical statements that I have described reflect the efforts of intelligent, creative scholars--persons who have been, at the very least, a step ahead of most of the rest of us. Nor do I have a conceptualization up my sleeve that I am confident takes us further. But I need to reiterate what I believe is a realistic appraisal: that what we now have represents attempts to capitalize upon apparent similarities between depressive symptoms and phenomena for which models existed elsewhere in psychology, and that in the process the affective aspect, though central to clinical notions of depression, has usually been treated as an afterthought. Seen in this light, I think it likely that history will view most or all of the theories of the seventies as first attempts, provocative but faulted.

In my remaining few minutes, I want to focus upon the canon implied in my use of the term "provocative." Theories are to be judged by the activity they stimulate: research activity and, in the case of psychopathology, therapeutic activity. In my view, the success story of the 1970s vintage theories is the substantial body of therapy research which it has generated, much of it since 1976. I have reviewed it elsewhere (Blaney, 1981) and Dr. Phillips may have more to say about it, but I have two general comments. First, there is real reason to believe that the new cognitive and behavioral therapies are indeed effective with depressives. Second, efforts to demonstrate that the source of the effectiveness rests in the process addressed by the instigatory theory are usually unsuccessful. Sometimes the therapies are generally effective while having no particular impact on the specific symptoms targeted in the therapy. These therapy studies have thus provided support for the theories which inspired them which is best described as highly ambivalent.

Turning to the less applied, more etiologically oriented research inspired by these theories, my general feeling is one of some disillusionment. Here I am trying to get as much distance as possible and specify what we really should be expecting of research in this field, knowing well that I have myself done research which does not meet these expectations and may do more of it yet. The psychological research on depression is easily flagellated on the basis of a few obvious shortcomings, of course--use of subclinical samples, unreal manipulations, ad hoc dependent variables, failure to subcategorize depressives, etc. But I want to focus on a broader issue which relates to the need for research studies to take into consideration the degree of complexity of the phenomenon being studied. Specifically, I believe that any reasonable theory must assume that a clinical depressive episode may derive from the person, the situation and/or the person situation interaction. We don't have time for it now, but it's not difficult to restate existing theories in this framework at least if a little common sense is thrown in as a supplement. The need for theories to be cast in this framework is, I believe, implicit in what has emerged from the most significant atheoretical body of research in the decade: that relating life stress to depression. Life stress, even when assessed carefully (e.g., Brown, 1979), accounts for some but not all the variance and it does interact with other variables. The remarkable thing about this interaction between situation and person, between adversity and vulnerability, in the causation of depression is that it is so intuitively probable that one would hardly need to have supporting data to argue that it should be a presumption around which any theory of depression is structured. Nonetheless, most of the theory-based research can be said to have looked only for main effects, and the theories have made it easy for this to be the case by virtue of their failure explicitly to state what the person predictions are, what the

interaction predictions are. In the published literature, the perspective I am espousing is typified most clearly by the following statement:

> In some depressives, chronically low self-esteem seems to operate like a classical trait disposition: the cognition-affect is broadly transituational and relatively independent of identifiable events. In other depressives, diminished self-esteem is triggered by a broad range of situations so that low self-esteem acts like a widely generalized stress response. In still other depressives, there seem to be complex interactions between self-esteem and particular events: in some, these stressors elicit a generalized deflation in self-esteem, whereas others experience a circumscribed deflation. Unfortunately, there are no data to substantiate the validity of these clinically derived distinctions (Becker, 1979, p. 317).

Briefly, I can state what I believe are several characteristics that useful theory-based research should have, and conversely, that theories should be phrased to provoke.

First, one must study persons who are not now depressed but who later become depressed, and one must be in a position to identify the chronic and transient personal characteristics which conferred a vulnerability, the stressful events which contributed, and the interaction between the two. The key point is that one must have some assurance that one has monitored these variables in the absence of depression itself.

Second, when doing this, ideally one should distinguish between those stressful events which are independent of personal characteristics and those that are not. Here, I am getting at the fact that some persons may have behavioral styles which tend to create events which are then stressful to them. And such person-fostered stress should be distinguished from other

stress. At the very least, one can separate events in which the person probably had no role, such as accidental death of spouse, from those in which he or she probably did have a role, such as divorce. Obviously, it is often hard to know who is to blame when things go wrong, however.

Third, when considering personal characteristics which confer vulnerability, one must distinguish between those which increase the likelihood of depression from those which merely increase the likelihood of a depressive diagnosis. For instance, imagine that in a longitudinal study you show that nondepressed persons who are pessimistic are more likely to become depressed than those who are optimistic. This may seem to indicate that pessimism confers vulnerability. But it may be that pessimism only increases the incidence of a beleagured person having the diagnosis of depression bestowed upon him. This error is risked if pessimism is treated as an indicator of depression. Conversely, the error may be avoided by making very sure that, when evaluating the predisposing power of pessimism, the definition of depressive outcome is divested of any consideration of the person's pessimism. This may result in the use of somewhat eccentric operational definitions of depression, and perhaps even in the use of several different definitions of depression in the same study, but I believe that it is essential.

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